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Department of Chemistry

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Dear Francis,

This is a combined reply to your letters of July 15(I) and August 3(II). I was in the process of finishing my comments on (I), when (II) arrived.

I am not surprised that you were not surprised (II) that I felt I had to reply to Nature's remarks. Not Nature, actually, but some masked reporter. What a stomach-centered creature, with his custard pie and hot potato! He seems to have a gustatory hang-up, which perhaps compensates for his ignorance of diffraction, among other things.

I gather from (II) that Nature has agreed to publish a considered article by you, which is certainly preferable to a hasty letter. If this is to contain the points at issue as you see them, i.e., that non-crystallographic dyads cause numerous reflections to be effectively centric and that in centric structures my arguments have much less force, it would be only correct if the same issue contained my point of view. Inasmuch as I am in no position to ring up the editor, as you have done, to make the appropriate arrangements, why don't you explain the above to him? I just happen to have a manuscript on this subject which constituted the first half of my first paper to Science, but which was scissored by the referees, who wanted the emphasis on DNA, and not Fourier analysis, contrary to my original intention. It wouldn't take too much trouble to make it suitable for a short Nature article to accompany yours.

However, I see no reason why you should quote my opinion in your paper. I would say that my opinion rightly belongs in a paper authored by me, while your papers should contain your own conclusions, based on your experience and knowledge of the literature.

To return to (I), most of it is really not concerned with Fourier analysis, and is thus not germane to what I have been talking about. What Khorana did, and what the evidence is for how the chains run, etc., has nothing whatever to do with the calculation of $\rho(xyz)$ and why that calcu-

lation, in this case, doesn't prove anything.

It is interesting that you have established that I do not have an adequate grasp of helical diffraction theory. You are not aware that there is no such thing as helical diffraction theory. This "theory" doesn't tell us one whit more than the expression $F_{hkl} = \sum_i f_i \exp 2\pi i (hx_i + ky_i + lz_i)$, which has been known for quite some time. The "theory" merely provided a short-cut, in precisely the same way Knott's molecular structure factor method did, but no one talks about molecular structure factor theory.

Finally, I see no reason for you to persuade the King's group to make available to me data which they should have published in the first place. It amazes me that some people consider their structures "generally accepted". As my second mentor in crystallography, J.H. Sturdivant, often said, "publish your data, for without them your structures can only be accepted on faith, which has no place in science". So much for dogma, which hasn't been doing so well lately.

Yours ever,



Jerry Donohue

JD:pm

cc: Mr. John Maddox